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45. δ Equulei. Components 5, 5·5; yellow. Hussey finds mass of system = 1·89 × Sun's mass and parallax = 0″·071, from spectroscopic measures.

46. τ Cygni. Components 4, 10. Burnham thinks the orbit doubtful and suggests that the companion is double, and on this hypothesis he has based his orbit.

47. κ Pegasi. Components 4·5, 5·0; yellowish. The brighter component is a spectroscopic binary. From the spectroscopic measures of 24·8 miles a second and period of six days I find a probable mass of the system = 0·48 × Sun's mass, and a parallax of 0″·106. This would reduce the Sun to a star of about 4·87 magnitude.

48. 85 Pegasi. Components 6, 10; yellowish, bluish. Brunnow found a parallax of 0″·054. This, with See's orbit, gives the mass of the system = 7·77 × Sun's mass. From measures of the binary and a distant 9th magnitude star Comstock, using a parallax of 0″·04, finds a mass of 11·3, the mass of A being 4·3 and that of B = 7·0 in terms of the Sun's mass = 1. With reference to this "remarkable" result he says, "A star A whose spectrum is of the second type (E in the Draper Catalogue) emits more than 100 times the light of its companion B, although B is presumably of equal age with A, and possesses 60 per cent. more mass than the latter star" (*Astrophysical Journal*, vol. xvii. p. 223). The companion is possibly gaseous. 85 Pegasi has a large proper motion of about 1″·3 per annum in the direction of position angle 140°.

On the Relative Efficiency of Different Methods of Determining Longitudes on Jupiter. By A. Stanley Williams.

I have read Professor G. W. Hough's paper "On the Determination of Longitude on the Planet *Jupiter*" in the Supplementary Number of the *Monthly Notices* very carefully, but fail to see any reasons for modifying the conclusions come to in my previous communication on this subject, in the March number. It seems to me that much of what he says has no direct bearing upon the subject and only tends to obscure the real questions at issue, and there are, moreover, several misstatements and misconceptions on his part. The position is simply this: In my previous communication I discussed in a particular manner not merely a few selected observations of a limited number of spots, but a very large amount of work done by Professor Hough with the micrometer upon numerous spots, and in addition I included his fine series of observations of the red spot, made when that remarkable object was at its maximum plainness. The first was to show the degree of accuracy attained by the micrometric method in general work, and the last what could be accomplished under the most favourable circumstances. I also discussed *in exactly the same manner* a correspondingly large number of observations of many different spots by the method of transits, made by various observers, and I came to the conclusion, as the result of this *exactly similar comparison*, that the apparent errors of the observations are about the same with either method, or, in other words, that the two methods

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yield practically results of equal accuracy.* Now in order to prove that this conclusion is erroneous it is necessary to show either that the mode of discussing or comparing the observations employed by me unduly favours the method of transits at the expense of the micrometric method or else that the selection of observations by the former method made by me is not a fair one, and hence does not really represent the average work of careful observers. And I submit that Professor Hough has not done either of these things, or in fact even weakened in the slightest degree the force of my conclusions.

For firstly, with regard to the mode of discussing the observations, it matters little if at all for our purpose whether we take the "probable error," or the "mean error," or the "mean residual," or any other form of expressing the apparent "error," or of indicating the accordance of the observations. The only point of real importance is that *exactly the same kind of "error"* must be used in discussing the observations made by both the two methods in order to properly and fairly compare the results obtained, and this is what I have done in my previous paper.† I selected for the purpose in view not the "average residual" but the "mean error" of an observation as defined in § 26 of Airy's work on *The Theory of Errors of Observations*; and every result included in my paper is expressed in this form of "error," with the single exception of the first one in the list on p. 433 of the *Monthly Notices*. I am perfectly aware that Schmidt gave ±1$^{\mathrm{m}}$·9 as the mean error of his corrected observations of the red spot, instead of ±1$^{\mathrm{m}}$·5, as stated in my paper; but the former is not the "mean error" as defined by Airy in § 26 of his work.‡ If Professor Hough will work out the "mean error" of Schmidt's corrected observations in the manner described by Airy on p. 19 of his book above cited, I think that he will find that it is ±1$^{\mathrm{m}}$·5, as stated in my previous paper.

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* The figures given by Professor Hough on p. 824 (paragraph 4) of his paper, it should be remarked, are not my figures, nor are the conclusions my conclusions.

† I should have thought that this must be obvious to any one, and there cannot be two questions about the matter. Professor Hough apparently considers that I had accused him of deriving the errors in his 1882 Report in an unusual or incorrect way. This is not, however, the case. What I disputed, and what I dispute now, is the legitimacy of comparing the very small errors found by him in a particular way with the necessarily much larger errors derived for the method of transits, or for that matter which would be derived also for the micrometric method, in a different manner altogether.

‡ The same remark will apply to the mean errors of Dr. Lohse's observations, referred to on p. 825 of the *Monthly Notices*. There are reasons why the Potsdam observations might be expected to show somewhat larger residuals, for they were not always made in the same way. Sometimes a micrometer was used, sometimes not, whilst a few observations by other observers (Kempf and Müller) were included. They do not form a perfectly homogeneous series. Some of the observations do not seem to have been central meridian transits at all. The mean error, as previously defined, of Schmidt's uncorrected observations would be less than ±3$^{\mathrm{m}}$·0.

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The same thing will apply to Professor Hough's "corrected" value for Professor Barnard's observations of the red spot, 1901, on p. 824 of the *Monthly Notices*, and to other differences instanced by him on that page, though I do not understand how some of his figures have been arrived at.* The mean error of Jedrzejewicz's observations is certainly $\pm 0^{m}\cdot 5$. It is doubtful if the use of a micrometer in the manner employed by this observer is of any advantage. In every case, with the single exception referred to above, the values given in my lists are correct, and Professor Hough is not justified in altering them.

Secondly, as regards the selection of the observations by the method of transits, Professor Hough affirms that "the table of so-called data consists almost entirely of the comparison of five or six observations *inter se*, selecting such as show a small mean residual." The first part of his statement, so far as it is correct, applies also to the results of his own micrometric observations. The last part of it is, however, quite incorrect. What I actually did was to search through all the likely periodical literature readily accessible to me for a good many years back, and I included in my lists and published *every instance* come across in my search, except as mentioned below, *entirely regardless* whether the residuals were small or large. The only cases rejected were, as mentioned in my paper, a few in which there were only three or (in one case only) four observations of a spot, and where consequently the mean error could not be ascertained with any exactness. I submit, therefore, that the data published in the lists in my paper represent *the fair average work* of careful observers in general by the method of transits, particularly if the adverse circumstances referred to in § 12 of the above-mentioned paper are borne in mind. The observations were certainly not selected on account of the smallness of the residuals. Is Professor Hough quite correct, though, in saying that "Barnard, without any investigation, *thought* some of his observations would have a mean error $\pm 0^{m}\cdot 7$"? In *Pub. A. S. P.*, vol. i. p. 91, Professor Barnard makes the definite statement that from twenty-three transits of the red spot "I get for the transit of the middle from the mean of the nine observations the error of the transit $= \pm 0^{m}\cdot 7$." I did not, however, as stated by Professor Hough, incorporate Barnard's data in my table, but assumed a larger mean error ($\pm 1^{m}\cdot 0$). With this single

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* For instance, Professor Hough states (p. 824) that my result for the red spot 1900 "carries a mean residual $\pm 2^{m}\cdot 8$, *not* $\pm 0^{m}\cdot 8$." But the actual residuals given by my observations are, $-0^{m}\cdot 2$, $-0^{m}\cdot 1$, $+1^{m}\cdot 2$, and $-0^{m}\cdot 9$ (*A. N.* 3675). The mean error is $\pm 0^{m}\cdot 8$, as correctly stated in my paper, the mean residual being $\pm 0''\cdot 6$. Professor Hough's figures for the mean error differ so frequently from mine that some additional data contained in his lists on pp. 828 and 829 of the *Monthly Notices* cannot possibly be accepted as comparable without examination. As this would entail the calculation of the rotation periods of the spots and the computation of the residuals shown by the observations this work must be deferred.

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exception the values given in my lists are correct and cannot be questioned.

It doubtless would not be difficult to select from the large number of published observations by the method of transits, by observers good, bad, and indifferent, and sometimes with quite small telescopes, instances of large or apparently variable errors, and this no doubt will account for some of the discordances that have been referred to. Even in the case of the comparatively small number of published micrometer observations it is not difficult to pick out such instances. For example, the mean error, as previously defined, of Professor Hough's twenty-five observations of the red spot in 1894–5[*] is as large as $\pm 3^{m}\cdot 5$, and there are residuals as great as $-10^{m}\cdot 0$ and $+11^{m}\cdot 6$. In other words, there is an extreme difference of $21^{m}\cdot 6$ in the time of transit of the red spot as deduced from the micrometer measures! It should be mentioned here that I purposely omitted this result from the list on p. 431 of the *Monthly Notices*, because it seemed to me that it would not be fair to the micrometric method to include it, since Professor Hough remarks that the observations were "comparatively rough, for the reason that the spot was often too indistinct to see any definite outline." On the other hand I did include the comparatively large mean errors for my own observations of this spot in the years 1898 to 1902, because I did not wish to reject a single instance come across in my search above alluded to, in which the observations were published in sufficient detail to enable the mean error to be readily ascertained, notwithstanding that the spot was then not only exceedingly faint, but there was the added difficulty that the altitude of *Jupiter* was very low. This is not the only instance in which the micrometric method was knowingly unduly favoured at the expense of the method of transits. But selected instances really prove very little, and there is now sufficient material available in order to compare general results.

With regard generally to the question of the inclusion of results derived from spots observed on a few nights only, to which Professor Hough makes objection, if we had a sufficient number of numerously observed spots we could dispense with them. An extremely small mean error—less, say, than $\pm 1^{m}$—is in such cases no doubt to some extent fictitious or accidental, but other spots similarly observed on only a few occasions may quite obviously give similar fictitious or accidental *large* mean errors. But by taking the average result of a sufficiently large number of *spots* the effect of such fictitious or accidental errors will be eliminated; in the long run the fictitious small errors will be balanced by the fictitious large ones. That this has been the case very nearly in the present instance will be apparent if we make a little analysis of the data in the March number of the *Monthly Notices*.

[*] See *A. N.* 3354.

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If we divide the data in question into two classes, treating all spots with less than ten observations as having a small number of observations, and those with ten or more observations as having a large number, we shall get the following figures:

Average Mean Error. Method of Transits.

	Small No. of Obs. m	No. of Cases.	Large No. of Obs. m	No. of Cases.
Red spot	± 1·8	3	± 1·7	13
Other spots	± 1·9	12	± 2·0	6

Average Mean Error. Micrometric Method.

	Small No. of Obs. m	No. of Cases.	Large No. of Obs. m	No. of Cases.
Other spots	± 1·8	10	± 2·5	5

In forming the first result I have omitted the first item in the list on p. 433 of the *Monthly Notices*, as this has been questioned. It will be seen that the spots with only a small number of observations give very nearly the same average mean error as the spots with a large number of observations so far as the method of transits is concerned. But—and this is somewhat curious—in the case of Professor Hough's micrometric measures the average mean error from spots with only a few observations is decidedly smaller than that shown by the spots with a large number of observations. In other words, the effect of including the spots with a small number of observations has been to *reduce* the magnitude of the average mean error in the case of the micrometric method, so that Professor Hough in objecting to the inclusion of such instances has actually been unwittingly damaging his own case! So far as the method of transits is concerned it makes practically no difference if the spots with only a small number of observations are omitted. It is interesting to contrast the position taken up here by Professor Hough with respect to fictitiously small errors deduced from a few observations with his treatment of the small residuals shown by his few *Saturn* observations in the *Monthly Notices* for January and April of this year. Yet the probabilities are vastly greater that the small residuals of the latter are "fictitious" than they are in any of the cases included in my lists, even if it were certain that all his observations related to the same spot.

Surely Professor Hough is not speaking seriously when he says (p. 825) that "if Schmidt's corrections were valid . . . the corrected observations can no longer be regarded as eye-estimates"? I cannot believe that this is really what he meant to say. No doubt these corrections are of an empirical nature, but they certainly do diminish the magnitude of the

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residuals considerably, whilst the observations are numerous enough to give grounds for accepting them. I quite fail, however, to see why a constant error varying with the position of the planet should necessarily "show the untrustworthy nature of eye-estimates." Surely not if we can ascertain its value at any time. Can Professor Hough point out any instance of a similar correction varying with the hour angle and affecting the observations of any other observer? It would seem that if the explanation he suggests for the origin of this error is correct, then its effects, so far at any rate as regards its variable part, should be eliminated by always observing with the eyes parallel to the belts. In this connection it seems proper to inquire whether Professor Hough always makes his measures with his head in the same invariable position with respect to the belts? Otherwise the experience of those engaged in astrographic work would seem to show that appreciable errors might creep in. It seems desirable that Schmidt's observations should be rediscussed from the point of view of a rotation period varying slightly with the time, in the manner adopted by Professor Hough in reducing his own observations. Such a rediscussion might materially reduce the apparent errors of the uncorrected observations and throw additional light on this correction question. Schmidt seems rather to have tried to force his observations to fit in with a uniform period.

As already stated selected instances prove very little, and they cannot modify the conclusions derived from the comparison of a mass of material, such as that on which those contained in my previous paper are based. On p. 827 of his paper Professor Hough has considered more particularly my observations of the red spot in 1887 in conjunction with others made by Mr. Denning during the same opposition. He appears to consider these observations as showing that the method of transits is not capable of yielding results of precision. It seems to me, however, with all deference to his opinion, that the only things really important brought out by the comparison is the danger that may arise from combining the observations of two different observers, and how unfair it may be to one observer to discuss his results mixed up in this way with those of another, so that I welcome this opportunity of considering the matter further, especially since it affords a good illustration of the small importance usually possessed by selected individual instances.

In the first place I would point out that nearly all my observations are weighted with the lowest possible weight 1 (on a scale ranging from 1, bad, to 5, good)—that is, they were expressly stated to be *bad observations.* In fact the seeing in the spring and summer of 1887 was often very unsteady at Brighton, and this, combined with the faintness of the red spot, and in particular of its preceding end, rendered it difficult to secure satisfactory observations of its transit with the 6½-inch

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telescope used.* These observations of mine are, therefore, no more adapted to indicate the accuracy attainable by the method of transits than are Professor Hough's own observations of 1894–5 so suited with regard to the micrometric method. Next, in order to show the unfairness of discussing the observations of one observer mixed up with those of others, I give below my own observations apart from those of Mr. Denning. The weights on the scale, 1–5, have been added from p. 96 of my *Zenographical Fragments*, but otherwise the figures have been taken from Professor Hough's paper, excepting that the residuals O – E have been determined afresh.

	Williams Longitude.	Wt.	O – E.
1886.	m		m
Dec. 20	– 11·0	1	+ 2·0
1887.			
Jan. 1	– 6·0	1	+ 7·0
Feb. 26	– 4·8	1	+ 8·2
Mar. 20	– 3·9	1	+ 9·1
„ 27	– 6·9	1	+ 6·1
„ 29	– 15·8	1	– 2·8
Apr. 3	– 9·9	2	+ 3·1
„ 10	– 21·8	1	– 8·8
„ 17	– 12·7	2	+ 0·3
„ 20	– 13·8	3	– 0·8
„ 24	– 15·2	2	– 2·2
„ 27	– 14·9	1	– 1·9
„ 29	– 17·9	1	– 4·9
May 2	– 15·6	1	– 2·6
„ 14	– 15·1	1	– 2·1
June 19	– 21·0	1	– 8·0
	Mean error =		± 4·4

	Denning Longitude.	O – E.
1886.	m	m
Nov. 23	– 2·5	+ 6·9
Dec. 17	– 4·9	+ 4·5
1887.		
May 7	– 15·5	– 6·1
„ 9†	– 15·5	– 6·1
„ 10	– 13·8	– 4·4
„ 26	– 14·8	– 5·4
June 10	– 11·3	– 1·9
„ 22	– 6·1	+ 3·3
July 16	– 5·8	+ 3·6
Aug. 6	– 4·2	+ 5·2
	Mean error =	± 4·7

The mean error of an observation is here $\pm 4^{\mathrm{m}}{\cdot}4$, not $\pm 4^{\mathrm{m}}{\cdot}7$, as given by Professor Hough on p. 827 of the *Monthly Notices*, or $\pm 4^{\mathrm{m}}{\cdot}9$ on p. 829, but it is evident that the observations are not well satisfied by Marth's ephemeris; there is a preponderance of plus residuals in the first half of the series and of minus ones

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* In *Popular Astronomy* for 1903, p. 69, Prof. Hough states that "my measures, when the spot was very indistinct, have been referred to the centre of the bay" of the S. equatorial belt. In light of this statement it seems probable that the Dearborn observations of 1887, published on p. 827 of the *Monthly Notices*, do not relate to the red spot at all, but to the deep bay or hollow in the S. equatorial belt—a different feature altogether.

† This observation is attributed to W. D. in the table in Professor Hough's paper, but it was actually made by D. W. only observed the following end of the spot.

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in the last half. A slight shortening of the rotation period corresponding to the ephemeris would considerably reduce the magnitude of these residuals, poor though the observations admittedly are.* It will be seen that *no less than* 12 of the observations have the lowest weight 1, and also that the four observations with higher weights give much smaller residuals, the mean error in fact of these four observations being only $\pm 1^{m}\cdot 6$, slightly less than that given by the Dearborn observations.

Mr. Denning's observations are given separately on the right side of the above table. The mean error is here $\pm 4^{m}\cdot 7$, but with regard to these observations it should be remarked that the first two must have been made under most disadvantageous conditions, when the planet was quite close to the Sun and its altitude very low. So probably were the last two or three observations.† Now there is a simple and it seems to me good reason why these observations made under such unfavourable circumstances should show plus residuals, and this reason is the faintness of the preceding end of the red spot in 1887. This is referred to on p. 96 of my *Zenographical Fragments*, and it was there pointed out that, owing to this peculiarity, there would be a tendency for the apparent centre of the spot to shift towards the following end in confused seeing; in other words, the transits might be observed later under such unfavourable conditions. In 1893 the spot had a somewhat similar appearance, and there is some evidence of a shift of this kind occurring in bad seeing.‡ If this view is correct, and it seems to me to be a reasonable one, we have a sufficient explanation of the occurrence of these plus residuals in the early and late observations. It is clear, moreover, that in this case if micrometer measures had been made by Mr. Denning at these times they likewise would have been similarly affected. The figures on p. 828 of the *Monthly Notices* quoted as given by me for the different rotation periods of the red spot in 1887 are quite correct, but it should be remarked that these figures were partly obtained by comparing the observations of two different observers, and they should be rejected.§ The Brighton observations taken alone do not indicate these supposed

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* With a somewhat shorter rotation period than that of the ephemeris the mean error can, by a graphical process, be reduced to $\pm 3^{m}\cdot 2$. The period of the ephemeris is $9^{h}\ 55^{m}\ 40^{s}\cdot 63$, but the rotation period of the red spot in 1887 seems to have been less than $9^{h}\ 55^{m}\ 40^{s}$. Professor Hough's observations would seem to make the period rather too long.

† Mr. Denning has himself called attention to the fact that observations made at such times are often necessarily of a rough or uncertain character, though it is useful to have them sometimes, owing to the long time interval they afford. See *Astronomical Register*, vol. xxii. p. 183.

‡ See *Memoirs B. A. A.* vol. iii. p. 139.

§ The writer would be extremely obliged if those who possess his little work *Zenographical Fragments* would kindly strike out the last lines on p. 96 referring to the irregular motion of the red spot, adding a reference to the foregoing.

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changes of period, neither do Mr. Denning's if the above explanation of the origin of the plus residuals in his early and late observations be accepted.* In any case a few discordant observations of a very faint spot made under what must have been very unfavourable conditions could not be held to prove them. Even when my book was written I was very doubtful as to the reality of the changes in question, and used the qualifying words "according to the observations" in referring to them. Professor Hough's remark that "here we have apparently a well-established fluctuation in the rotation period of 5·2 seconds which did not exist" therefore loses all sense. With my present knowledge of the different manner in which different observers may regard the same planetary marking I should not dream of coming to any such conclusion from the published observations, or from a comparison of the observations of two different observers, even if they related to a conspicuous spot. It is apparently from cases like the foregoing, based on the comparison of 16 observations of a very faint spot, 12 of which are clearly stated to be bad ones, by one observer, mixed up with 10 observations, some of which must have been of a very rough character, by another observer, and without making any adequate investigation himself into the circumstances, that Professor Hough largely bases his condemnation of the method of transits! His observations of the red spot in 1894–5 have already been alluded to, and I give them below for comparison with the above. They have been taken from his paper in the *Astr. Nach.* 3354, but the residuals O—E have been added by the writer.

	Hough Longitude.	O—E.
1894.	m	m
Sept. 26	+ 1·1	− 3·5
Oct. 10	− 5·4	− 10·0
Dec. 3	+ 9·1	+ 4·5
,, 8	+ 6·8	+ 2·2
,, 13	+ 11·2	+ 6·6
,, 25	+ 16·2	+ 11·6
,, 28	+ 4·3	− 0·3
1895.		
Jan. 2	+ 2·2	− 2·4
,, 9	+ 8·4	+ 3·8
,, 16	+ 1·8	− 2·8
,, 23	+ 5·4	+ 0·8
,, 26	− 2·6	− 7·2
,, 28	+ 0·5	− 4·1
Feb. 2	+ 3·1	− 1·5
,, 14	+ 6·1	+ 1·5
,, 16	+ 5·6	+ 1·0
,, 19	+ 1·7	− 2·9
,, 21	+ 4·9	+ 0·3
,, 23	+ 6·8	+ 2·2
,, 26	+ 8·9	+ 4·3
Mar. 5	+ 1·8	− 2·8
,, 10	+ 5·7	+ 1·1
,, 19	+ 9·4	+ 4·8
Apr. 3	− 0·7	− 5·3
,, 10	+ 3·9	− 0·9
	Mean error =	± 3·5

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* In answer to an inquiry respecting his observations of 1887 Mr. Denning writes that "my observations of the red spot in 1887 were very unsatisfactory, owing to the faintness of the object and to the bad definition

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As already stated the mean error of an observation is here $\pm 3^{m}\cdot 5$, whilst there are residuals amounting to $-10^{m}\cdot 0$ and $+11^{m}\cdot 6$. The grouping of the residuals is also interesting and instructive in comparison with the foregoing observations of 1887. Both at the beginning and end of the series are rather large minus residuals; whilst the curious way in which plus or minus residuals are repeatedly grouped together suggests that the observer several times in the course of the observations suddenly changed his habit of observing. If the first two observations had not been made a different conclusion might have been come to respecting the rotation period of the spot in 1894–5. I do not think that my observations of 1887 can be said to compare unfavourably with these ones of Professor Hough, which latter were only said to be "comparatively" rough. It is easy to see, moreover, that if only the two observations at the commencement and the two at the end had been made, with a few of the intermediate ones, the result might have resembled Mr. Denning's series pretty closely, with reversed signs.

Until we are able to compare contemporaneous series of observations by several observers using the micrometric method,* it seems to me idle and useless to enter into the question of personal equation, or of what Professor Hough calls "variable error," so far at least as regards comparing the efficiency of the results obtained by the two methods of observation is concerned. "Variable error of the method of transits" has a formidable look about it, no doubt, but in most of the cases instanced on pp. 829 and 831 it would be perfectly legitimate to change the signs and call it the "variable error of the micrometric method," since even with our present knowledge a similar error undoubtedly does sometimes affect observations made by the latter method. Both personal equation and this "variable error" beyond all doubt arise in great measure from different observers not observing irregular markings in the same manner, and I can see no reason at present for supposing that they would be smaller in the case of the micrometric method than they actually are with the method of transits. It is notorious how differently different observers see planetary markings, even the most prominent ones, and it is absurd to imagine that they would all measure them alike. Even in the case of such an unusually large, definite, and regular marking as the red spot when at its maximum plainness a glance through the numerous

usually prevailing. For these reasons my transits obtained in that year were about the very last I should have chosen as a fair example of accuracy in eye-estimated transits . . . they were some of the worst observations I have ever made."

* Since the appearance of a spot is dependent largely on the aperture of the telescope employed and the atmospheric conditions prevailing, such micrometric work ought to be undertaken not only by different observers at different stations, but also with telescopes of various sizes, before any fair comparison can be made with the results obtained by different observers by the method of transits.

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drawings of the period will show, I venture to think, that differences of the kind must necessarily arise. Some observers, for instance, draw the red spot at this time neatly rounded off at the ends; others show the ends pointed; others with the ends not only pointed, but drawn out into fine lines; yet others with the ends dissimilar; and so on. Dr. O. Lohse once saw the spot with the following end rounded off, but with the preceding end pointed and unsymmetrically flattened, the point being, moreover, extended into a fine line. I doubt if any two observers would agree as to what constituted the middle of such an object, the appearance of which might vary largely with the conditions of the seeing and the size of the telescope used. Professor Hough has himself drawn the following half of the red spot much broader than the preceding half. How would he measure such an object? Would he measure a point midway between the two ends, or would he measure what appeared to be the centre of figure of the spot? Again, a very common form of spot on *Jupiter* takes the shape of a dark mass projecting obliquely from a dark belt into one of the bright zones. Such spots are frequently exceedingly dark and definite at the projecting part, and from thence fade off gradually. Probably again no two observers would agree as to what is the middle of such a spot, and whilst some would no doubt try to observe the middle, or the spot, as one mass, others, including the writer, would probably observe the more definite little projecting part, though this might be far removed from the centre of the mass. On a poor night this more definite projecting part might be obliterated, and then the last-mentioned observers would necessarily observe the spot as one mass, or its apparent middle. This is probably one and a very common way in which what Professor Hough calls "variable error" arises; but micrometer measures would assuredly be affected in the same manner. It is not difficult to see how differences amounting to 5 or even 10 minutes in the time of transit might occur in this manner; and, as every spot may be said to have its own peculiarities of appearance, personal equation or "variable error" originating in this manner might be expected to differ in the case of different spots, as is actually the case. From the Dearborn observations of the red spot of 1894–5 it would not be difficult to pick out two or three "variable errors." Another instance of the kind may be referred to as occurring in Professor Hough's famous early series of observations of the red spot. In 1881 he observed the red spot in a pretty uniform manner for a couple of months up to December 28, when a sudden change seems to have taken place in his habit of observing, and during 1882 January he systematically observed the transits, as deduced from his micrometer measures, nearly 4 minutes earlier. These, and other cases that could be adduced, show clearly that, even according to our present knowledge, Professor Hough must be wrong in assuming that the micrometric method is free from this "variable error."

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Referring again to these Dearborn observations of 1894–5 of the red spot, it is possible that they may furnish the key to the origin of some part, though only some part, of this "variable error." They seem to indicate that perhaps observers may not always have been careful to distinguish between the centre of figure and the centre of mass, or intensity, of the red spot, and may sometimes have observed the one and sometimes—particularly in poor seeing—the other.

Much importance seems to be attached to the accordance of measures made on the same night before and after the central transit of a spot; but although this, if correct, would show that the same observer on the same night, and after the lapse of only an hour or two, may measure a spot in the same manner, yet it does not prove in the least that another observer, with another telescope and at another place, with different conditions as to seeing, must measure the same spot in the same manner, nor even the first observer on a different night and under different conditions. It has no bearing upon this question at all. Such accordance would indicate that the disc of the planet was exactly bisected, but would the measures show that the *spot* was similarly bisected? But I do not think that Professor Hough is justified in making the statement as to the accordance of the measures made before and after the transit of a spot. In the 1882 *Report of the Director of the Dearborn Observatory* particulars are given of the measures of the red spot. On six nights there are not less than two measures on either side of the central meridian. These give the following differences for the time of transit of the red spot:

		m			m
1880 July 22	Diff. =	+0·7	1880 Dec. 31	Diff. =	−2·0
Sept. 9	" =	−2·7	1881 Aug. 9	" =	+2·0
Nov. 1	" =	−0·4	Dec. 28	" =	+0·4

The *plus* sign signifies that the measures made when the spot was following the central meridian make the time of transit later than those made before the transit. It would seem from the above figures that differences amounting to two or three minutes in the time of transit of a spot may have arisen according as to whether the measures were made when it preceded or followed the central meridian. The material is not sufficient to decide if there is any systematic error, but the grouping of the *plus* and *minus* differences is suggestive of this.

On p. 830 of the *Monthly Notices* the very remarkable statement is made that "although the motion of no spot on *Jupiter* can be regarded as constant for a long interval, yet there is never any abrupt change in the drift, as indicated by my observations for the past twenty-five years." Can Professor Hough have forgotten his observations of a white equatorial spot in 1881–2 (see the *Report of the Director of the Dearborn Observatory*,

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1882, p. 51)? Concerning this spot he remarks that "during 1881 the single spot, observed continuously for a period of 252 days, indicated *sudden deviations* in its apparent place . . . the comparison with the ephemeris shows a maximum displacement of sixteen minutes of time."* The italics are mine. As a matter of fact it is chiefly in the equatorial regions of *Jupiter* that "sudden deviations" of this kind occur, according to observations made by the method of transits, and it can only be because Professor Hough has not apparently observed a large number of these equatorial spots in a sufficiently continuous manner that he has not frequently come across similar deviations revealed by his micrometer measures. These deviations or wanderings, as I have termed them, are familiar to every systematic observer of the equatorial markings. They were first pointed out, I believe, by Herschel in 1780,† and their existence led this distinguished astronomer to abandon the planet *Jupiter* for *Mars*, so far as the chief purpose he had in view at the time was concerned—namely, the question of determining whether the *Earth's* diurnal motion is perfectly equable.

The reason for my venturing to question the correctness of Professor Hough's identification of four observations of an equatorial spot (*Monthly Notices*, p. 833) is that the accordant observations of four different observers show that there were several similar equatorial spots near the same place, and his four observations fit in with three separate spots. As to the number of observations required to insure correct identification, opinions will, no doubt, differ, but it does not seem as though four observations of an equatorial spot scattered over an interval of eighty days is sufficient. In this particular case two of them moreover are close together at the end, so that practically the identification is dependent upon three observations, separated by thirty-seven and forty-one days. A glance over the observations of the spot referred to in the preceding paragraph will satisfy any one, I should think, that the identification might well be incorrect under such circumstances when there are several similar spots close together.

With regard to *Saturn*, since the rotation of this planet and that of *Jupiter* is performed in nearly the same time, the reduction in scale due to the greater distance and smaller size of the

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* I find that Mr. Denning had already called attention to this in the *Monthly Notices* for June last, p. 768.

† *Phil. Trans.*, 1781, p. 126. These deviations or wanderings occur in the case of the neatest and most definite markings, and they cannot possibly be confounded with any of the discordances of the nature of personal equation or variable error, for the following reasons: (1) They are attested by the accordant contemporaneous observations of several different observers. (2) They may cause differences in the time of transit of a spot so considerable as to amount to more than half an hour in a few days. (3) They may affect nearly simultaneously several adjacent spots, whilst other similar spots, in the same latitude but in a different longitude, at the same time remain unaffected.

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former planet must affect the accuracy of the results obtained by the micrometric method. The reduction in scale actually amounts to near one-half, and, since the Dearborn micrometrical measures of Jovian spots give an average mean error of ±2^m in the deduced time of transit of a spot, it follows that similar measures of spots on *Saturn* may be expected to have a mean error of about ±4^m. This is, I should imagine, a minimum value, and it might be nearly twice as large in the case of an individual spot. Is there any reason why the method of transits should undergo a greater reduction in accuracy than the micrometric method? The observations of Professor Barnard, quoted on p. 832 of the *Monthly Notices*, show clearly what an indefinite and difficult object the spot must have been, but would micrometer measures of such an object have given a more accurate result than a central meridian transit? I doubt it very much.

There are several other matters considered in Professor Hough's paper that ought to be gone into, but this communication has already reached to an undue length and their consideration must be deferred. Some of them do not seem to require noticing. I need hardly add that any conclusions come to in his paper, so far as they imply any superiority on the part of the micrometric method, are not warranted by the known facts.

I must confess that I have been surprised to find that the micrometric method does not yield results of greater precision, and this circumstance, taken in conjunction with Professor Hough's own admission that his micrometer measures of Jovian spots *are only half as accurate* as those of double stars,* seems to suggest that there may be some hitherto unsuspected source of error affecting the micrometric method. Possibly the placing of a micrometer wire over a spot may prejudicially affect its appearance to an appreciable extent? A very minute spot is apt to disappear altogether under such circumstances, as is also a larger one if very faint and indefinite. In many cases it would seem as if this must give rise to the possibility of error. For instance, if the middle part of an irregular spot is cut out by a "wire" placed over it, it would seem as though errors might readily be introduced, and that such errors might even be dependent to some extent upon the thickness of the micrometer wires, so as to be different to another observer using wires of greater or less fineness. They might even vary to the same observer with the change in the apparent diameter of the planet. On p. 829 of the *Monthly Notices* reference has been made to a curious instance of an apparent difference in longitude between two spots as deduced from transits observed by Professor Barnard compared with his direct micrometer measures of the distance separating them. It does not seem, however, to have occurred to Professor Hough that the origin of this difference may possibly be traced to the micrometer. He also seems to have overlooked the fact that Barnard in his transit

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* *Popular Astronomy*, 1903, p. 66.

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observations used a micrometer with one wire bisecting the disc, presumably set so as to indicate the measured half-diameter of the planet, so that Barnard's *central meridian* can hardly have differed in the sense implied. Concerning the direct measures of the distance between the two spots Professor Barnard writes that "although the spots were usually very conspicuous, it was found that they were quite ill-defined and rather difficult when the wires were placed over them."* This statement seems to support the suggestion made above as to the possible prejudicial effect of a micrometer wire in altering the appearance of a spot. Without a micrometer these spots were almost ideal objects for observation.

Hove: 1904 *November* 29.

On the Eclipse of Agathocles. By Simon Newcomb.

In my "Researches on the Motion of the Moon," which appeared in 1878, I made a careful study of the accounts of supposed total eclipses of the Sun by ancient authors, with a view of determining whether any of them could be used either as tests of the lunar tables, or as auxiliaries in the determination of the slow changes in the lunar elements. The conditions required for this use were that some determinable eclipse should have been total at a known place. The conclusion which I reached was strongly in the negative. Not only did there appear to be no ancient eclipse which we could conclude was really total at a given place; but the accounts were generally so vague that no interest seemed to attach even to a comparison with the lunar tables, except for chronological or historical purposes.

I may add, in all frankness, that these adverse views have not been shared by those astronomers who, in the meantime, have made researches on the subject. Oppolzer and, after him, Ginzel had such confidence in the reality of these eclipses as to use them as the basis of corrections to the elements of the Moon's motion which are incompatible with gravitational theory. I have no intention to argue my view at present further than to say that I am not at all convinced it was in any point ill founded, so far as related to data available at the time. But since my paper was published a very important point has been brought to light showing an exception to the conclusions there reached. This arises in connection with the eclipse of Agathocles —309 August 14.

This eclipse has been so fully discussed by Airy and others that only a very brief statement of the circumstances connected

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* *Monthly Notices*, vol. li. p. 549.